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Problems with Publishing: Existing State and Solutions

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Abstract

Survival in academia depends on publications in refereed journals. Authors only get their papers accepted if they intellectually prostitute themselves by slavishly following the demands made by anonymous referees without property rights to the journals they advise.

Intellectual prostitution is neither beneficial to suppliers nor consumers. But it is avoidable. The editor (with property rights to the journal) should make the basic decision of whether a paper is worth publishing or not. The referees should only offer suggestions on how to improve the paper. The author may disregard this advice. This reduces intellectual prostitution and produces more original publications.

Keywords: academic career, market for economists, publishing

JEL Classification: A11, Z00

Prostitution of ideas and academic career

This paper will never be published in a (refereed) economics journal, nor will it be submitted to a scholarly journal.

The author knows that if he submitted it, he would be lucky if, after something like a year or so, he got an invitation to resubmit the paper *according to the demands* exactly spelled out by the two to three referees and the editor(s). For most scholars, this is a proposal that cannot be refused, because their survival in academia crucially depends on publications in refereed professional journals. They are well aware of the fact that they only have a chance to get the paper accepted if they slavishly follow the demands formulated. The system of journal editing existing in our field at the present time virtually forces academics to become prostitutes: they sell themselves for money (and a good living). Unlike prostitutes who sell their bodies for money (Edlund and Korn, 2002), academics sell their soul to conform to the will of others, the referees and editors, in order to gain one advantage, namely publication. Most persons refusing to prostitute themselves and to follow the demands of the system are not academics: they cannot enter, or have to leave, academia because they fail to publish. Their integrity survives, but the persons disappear as academics.

This paper discusses the process forcing persons wanting to pursue a university career to act as intellectual prostitutes (Section 2). Intellectual or academic prostitution is defined here as acting against one's convictions in order to get a reward. The reason for such academic prostitution¹ is seen to lie in the institutions of journal editing currently existing in economics. This system essentially accords veto power to every referee.² A prospective

author therefore has to meet the demands formulated by the referees in order to have a chance of having the paper accepted for publication.

Individuals react in many different ways to the incentives to intellectually prostitute themselves. Many internalize the journal publishing system so completely that they do not consider that they commit such acts; others flatly refuse to partake in the system. But most academics pursue an intermediate strategy. They are aware of the moral cost involved, but are prepared to compromise.

Some readers may feel that intellectual prostitution is not morally objectionable *per se*, but simply a particular choice made by would-be authors. This is correct; prostitution can be looked at as a normal market activity without any moral connotation.³ The problem is that both the production and consumption activities going with academic prostitution produce undesirable outcomes. It is neither beneficial to the suppliers of academic prostitution nor to its consumers. A major reason for this failure to produce good results lies in the fact that the (anonymous) referees have no property rights on the journals they advise (Section 3).

Other readers may think that the kind of prostitution discussed here is unavoidable. The fundamental fact of scarcity forces all of us to adjust our behavior in order to survive. Suppliers wanting to sell their goods and services must carry out the wishes of the potential customers. Section 4 proposes a different point of view. Scholars are seen as performing a similar activity to artists, in particular painters who, since the Renaissance, are expected to express their own beliefs and convictions—which led to an explosion of creativity in the arts. The almost dictatorial demands advanced by the referees are difficult, or even impossible, to reconcile with authors wanting to publish their own ideas in economics journals.

The final Section 5 advances a modest proposal designed to substantially reduce the “need” to prostitute oneself in order to publish and be academically successful. It seeks to overcome the veto power of (anonymous) referees, whose interests are not aligned to those of the journal, because they have no property rights on the journal they advise, while the editor who does have property rights in the present setting is unwilling to overrule the referees. My proposal is simply that the editor in charge makes an initial decision whether a paper is worth publishing or not. The referees are only asked to give suggestions on how to improve the paper. The author is free to follow or to disregard this advice. While there may be some journals at least partly following this procedure, most refereed journals today certainly are not. They rarely, if ever, overrule the advice extended by the referees. Most journals get many more papers than they can possibly accept (rejection rates of over 95 percent are quite common) and therefore grant *each* referee veto power. Thus, an author must conform so much to the demands that acceptance by *all* referees is guaranteed. This applies to the decision of whether to reject or offer the possibility of revision in the first round, as well as to the decision to accept or reject a revision in a later round.

How academic prostitution evolves

The decision process inducing individuals to intellectually prostitute themselves may be captured by a game between the would-be author and the editor representing the journal. The extensive form of the *Journal Publication Game* is pictured in figure 1.

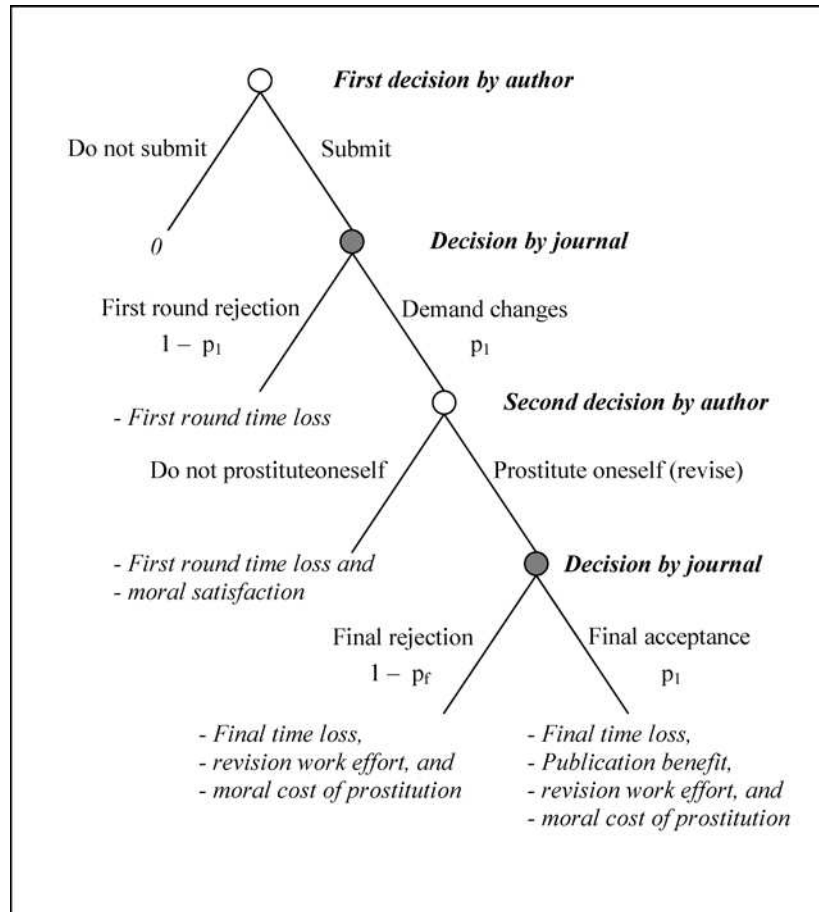


Figure 1. The journal publication game.

The author's first decision is whether he wants to submit an article to a scholarly paper or not. Only if he is willing to submit does he have a chance of eventually publishing the paper and entering and staying in academia. After considerable time has passed (today one year is not unusual⁴), the editor of the journal either rejects the paper in the first round or *demand the revisions found necessary by the referees*. The author's second decision is whether to make revisions according to these demands and therewith prostitute himself academically, or withdraw. In the latter case, the author has the gratification of keeping his intellectual purity, but time has been lost and the chance of a university career vanishes. If the author revises according to the demands of the referees,⁵ the editor takes the final decision⁶ of whether to reject or accept the paper for publication with a given probability. In the case of rejection, the author has expended considerable work effort to please the referees, has lost even more time (something like two years is not unrealistic), and has to

carry the moral cost of having had to prostitute himself. The effort, time and moral costs also apply in the case of acceptance, but they are counterbalanced by the benefits of having an article published, and therewith being able to enter or stay in academia. An author is more likely to intellectually prostitute himself (i.e. to revise) rather than withdraw, the higher the (subjectively expected) chance of final acceptance and the lower the (expected) revision work effort, the higher the cost of time for the revision and the lower his moral cost of prostitution.

This game captures the essence of the academic publication process as I see it.⁷ It allows us to focus on the crucial determinants of the process.

Revisions demanded

All authors would like to receive referee reports helping them to improve their paper. Alas, this is rarely the case. Normally, the referees want to see substantial changes basically altering Many economic scholars are likely to be in complete disagreement with this interpretation of the behavior of the referees. They like to think that the referees only ask for changes improving the paper in the interests of the author, but refrain from interfering any further.

Surprisingly enough, the (economic) literature on journal publication⁸ does not offer any theory about the behavior of referees. The implicit assumption is that referees act in the interests of science as a whole. Engers and Gans (1998), (reprinted in Gans, 2000: 140) explicitly assume that “referees are motivated by a concern for the quality of research.” The notion that individuals act according to general social interest is totally inconsistent with the traditional rational choice model of man (e.g. Becker, 1976; Frey, 1999).

No well-worked out theory on the behavior of editors exists. Laband and Piette (1994), (reprinted in Gans, 2000: 119) state: “to our knowledge, no widely accepted theory of editorial behavior has ever been articulated.” In the literature, similar implicit assumptions as those made for referees are current (see Vandermeulen, 1972; Laband and Piette, 1994).

A useful starting point for a rational choice theory of referees’ and editors’ behavior is to acknowledge the difference between the two groups of actors⁹ on property rights to journals. Anonymous referees have no property rights to the journal they advise. They are not concerned with the effect their advice has on the journal. The absence of property rights must be expected to lead to shirking. The interests of the journal and the referees are not aligned.

The referees find themselves in a classical low cost situation (Kirchgässner, 1992). Their decisions with respect to the evaluation of the papers in their hands has little or no consequences for them, provided they keep to the *formal* rules of the profession.¹⁰ But with respect to the *content* of the evaluation, they are free to do whatever they please. In a low cost situation, the referees attribute some weight to what *they* consider to be the “common good.” But it would be naive to assume that they only do that. Personal interests must also be expected to play a role. Many referees will be tempted to judge papers according to whether their own contributions are sufficiently appreciated and their own publications quoted. They carry, for instance, no costs when they advise rejection of a paper they dislike (e.g. because it criticizes their own work), even if they expect that it would be beneficial for economics



Figure 2. Extent of property rights to journals.

as a discipline. It is important to recognize that no “bad” behavior is subsumed here, but simply a behavior in line with what economic theory assumes as a matter of course in all other areas (for instance with respect to the firm, Milgrom and Roberts, 1992; Gibbons, 1998; Prendergast, 1999). Cognitive dissonance reduction (Festinger, 1957) ensures that, in most cases, the referees do not perceive any conflict between what *they* consider to be the “common good” and their own private interests.

In contrast to referees, *editors* enjoy property rights to “their” journal. The editors’ reputation is enhanced by the quality of their journal even if they do not own it. In contrast to the referees, editors are not anonymous; rather, their names are well known in the profession and are closely connected with “their” journal. Well-known examples are Keynes and the *Economic Journal* and Stigler and the *Journal of Political Economy*. The property rights of editors work indirectly through their reputation. As a consequence, editors are interested in the influence and quality of their respective journals. Such effects of reputation, and therefore the extent of property rights, are the weaker, the larger the group of editors is. Scholars outside the United Kingdom would, for example, find it difficult to name the five managing editors of today’s *Economic Journal*. The extent of property rights to a journal thus varies according to organizational forms and actors, as illustrated in figure 2.

Complete property rights are owned by a journal edited by its owner, but this organizational form does not seem to exist in economics (at least not with any well-ranked journal).¹¹ A sole, and therefore well-identified, editor owns stronger implicit property rights than a group of editors. When referees are identified (for instance by putting their names at the beginning of a published article¹²), their reputation is to some extent connected with the journal. Anonymous referees have no property rights in the journals they advise. Therefore they must be expected to act least in line with the interests of the respective journals. Following economic theory, the more one moves to the right in figure 2, the stronger the private interests of editors and referees to reveal themselves is to be expected.

Some authors may subjectively perceive that all that the referees ask is also in their own best interests and in no case leads to a distortion of their own thinking. But such perception can also be viewed as the result of having so much internalized the existing publication process that even the idea of intellectual prostitution is alien. This is a striking case of a reduction in cognitive dissonance (see Festinger, 1957, in economics Akerlof and Dickens, 1982): those individuals prepared to follow the demands of the referees to the letter experience less personal cost if they can convince themselves that this in no way distorts their own thinking.¹³ Viewed *ex ante*, however, intellectual prostitution remains an issue.

Cost of time

Authors experience the opportunity cost of time throughout the whole publication decision process. It becomes particularly visible when the paper is rejected in the first or in a later round because then it is not counterbalanced by any potential or actual benefit of publication. The size of the time cost varies according to institutional conditions and one's career achievements.

In a competitive academic system, such as in the United States, and increasingly also in Europe and elsewhere, graduate students and assistant professors are under extreme time pressure. In some cases, it is an "all or nothing" issue: either they are able to publish in a good refereed journal, or they have to bury their dream of an academic career. The number of publications determines the ranking of the university offering a position. Getting off to a good start is important,¹⁴ as it is quite difficult to move upwards once one has left top universities. But the time available as a graduate student or assistant professor is generally severely limited, so that a time loss is a serious matter. It follows that for scholars starting a university career, the time costs are high, which *cet. par.* raises the option of not submitting the paper and looking for an alternative occupation. But this group of people also has particularly high benefits from publishing in a refereed journal, which raises the attractiveness of submitting. A rational solution is to work hard and intensively when young, raising the productivity of a given period of time. This is indeed what is commonly observed: young scholars in competitive academic systems work extremely hard and long hours. In contrast, young scholars in the classical European university system tend to focus less on their narrowly defined discipline.

For more advanced scholars, the time cost of journal publication is lower. The cost side thus provides an incentive to submit more to the journal publication process. However, in a university with muted competition (such as existed in the past, and still exists to some extent, in Europe), there is little or no benefit from publishing in refereed journals. In that system, few tenured professors submit to refereed journals and take the trouble of revising their papers. In contrast, in a competitive academic system, even for advanced scholars, the current journal output matters considerably.¹⁵ Empirical evidence (Oster and Hamermesh, 1998) suggests that, on balance, the lower benefits of journal publishing at a higher age outweigh the lower cost and that therefore older researchers submit less papers to journals.

Moral costs of intellectual prostitution

The utility loss experienced by scholars¹⁶ who are confronted with undesired demands for revisions by referees depends on various factors. It is useful to distinguish between two extreme types:

- (a) An "autonomous person" with a strong identity has well-developed ideas of his own and therefore suffers high costs from intellectual prostitution. It may be hypothesized that older and more successful persons are more likely to belong to this category than young and less successful ones. As a consequence, the former are less likely to engage in the publication process and pursue an academic career. Young scholars, in contrast,

may find it less taxing to yield to the demands of referees and therefore are more likely to submit papers to refereed journals.

- (b) “Other-directed persons” have low costs to adjust to the demands of other persons. This may be due to their genetic inheritance (these might be called “*born intellectual prostitutes*”) while others have learned the need to adjust in order to be able to publish (they might be called “*learned intellectual prostitutes*”).

Some of the born or learned academic prostitutes have reached a high level of competence. In particular, they are masters at predicting what the referees and editors want to see, and from the very beginning introduce it into their papers.¹⁷ The extent of prostitution can then no longer be identified by looking at the changes undertaken in the course of the publication process. The more perfectly the authors are able to anticipate the demands, the less they need to change. Due to the lower cost of meeting the demands for revision, born and learned intellectual prostitutes are more likely to engage in the publication game and to stay in academia.

A comparison between autonomous and other-directed persons suggests that the members of a competitively oriented academia tend to be more malleable and more directed towards fulfilling what they see to be the prevailing standards. In university systems where those inside the system have privileged positions, a larger number of more original persons (but many without much outside influence) may be found.

Benefits of publication

Rankings of individuals, departments and universities in modern economics are based to a large extent on publications in refereed journals (see e.g. Graves, Marchand and Thompson, 1982; Laband, 1985; Dusansky and Vernon, 1998). Publications in such journals provide substantial benefits in terms of career, income and internal recognition (see Tuckman and Leahey, 1975; Hansen, Weisbrod and Strauss, 1978; Hamermesh, Johnson and Weisbrod, 1982; Diamond, 1986; Saurer, 1988). As already pointed out, scholars at the beginning of their careers derive larger benefits than already established scholars, and therefore are more inclined to undertake journal submissions.

In competitive academic systems, citations have become increasingly used as a ranking device (see Blaug, 1999; Frey and Eichenberger, 2000). This reduces the importance of journal publications because citations refer to any kind of “publication”, including papers put on the web. Some working paper series have started to introduce a refereeing system also for web publications, so that the costs caused by intellectual prostitution are transferred to this publication outlet. In principle, however, anyone can put their papers on the web without having to go through a refereeing process. While attention will be smaller than in well-established official working paper series, it nevertheless opens an effective way of evading intellectual prostitution. If the competition of ideas is really as open as many people hope it is, the higher level of original and unconventional ideas published on the web may enliven and benefit economics.¹⁸

The discussion of the publication decision process has shown that there are various important determinants affecting the likelihood of submitting and revising a paper, and

therewith accepting academic prostitution. The weight of these determinants depends not only on personal characteristics (“type of person”) but most importantly also on the existing academic system (especially the extent of its competitiveness) and one’s position on the career ladder. Young scholars beginning to engage in a university career in general have lower costs and higher benefits from submitting and revising papers. They are more prone to engage in intellectual prostitution than are their more established (and often tenured) colleagues. As has already been made clear, this has little or nothing to do with “morality”, but is due to their more favorable expected net utility of publishing in refereed journals.

Intellectual prostitution and no fun

So far the cost of conforming to the demands of the referees for the would-be authors has been analyzed. But what about the consequences of imposing the refereeing system for prospective readers of the articles? Perhaps the cost of intellectual prostitution to the authors is fully compensated by the benefits to the readers.¹⁹

The initial purpose of the refereeing system was to select or screen the “best” papers. Only gradually has it evolved into a “censuring” system, making it most difficult to have unconventional ideas accepted. Consider the case of three referees, each having veto power. The probability of an unconventional idea not being vetoed by any of them, nor by the editor(s), is very small because it is highly likely that one of them dislikes a new thought for one of three reasons:

- (i) the idea is new and therefore more difficult to grasp and appreciate for the referees than are more conventional contributions;
- (ii) the referees are normally leading researchers in the topic treated (see the evidence in Hamermesh, 1994). They fear the loss of some of their reputation if a new idea is introduced;
- (iii) the new idea is less well formulated than are the well-established ideas and therefore is rejected for lack of rigor.

For these reasons, the present refereeing system tends to work against originality, but it may still make a good job of choosing the “good” papers. It is, of course, not possible to state in an absolute way what a “good” paper is. It is only possible to evaluate whether a paper corresponds to the generally accepted criteria which have emerged in a discipline. Circumstantial evidence lends some support to the notion that the present system of academic journal publication does not lead to beneficial outcomes from a broader point of view (except in part for those who have made it in the profession²⁰):

- (1) Many authors state that the refereeing process has indeed helped them to improve their papers. According to an empirical analysis undertaken by Laband (1990), referees “add value” to papers.²¹ But more importantly in our context, they also often confirm that in order to have their papers accepted, they were “forced” to delete those parts of the paper particularly dear to them. Many authors feel that the refereeing process robbed them of the chance of really contributing what they find important and innovative.

An example is given by Brian Arthur, who states that “I put the paper (“Competing Technologies, Increasing Returns, and Lock-In by Historical Events,” finally published in the *Economic Journal* 1989) through eight rewrites in this (revision) process; each time it became stiffer, more formal, less informative, and as a result more publishable” (Gans and Shepherd, 1994, reprinted in Gans, 2000: 35). Sometimes the papers published reflect more the referees’ than the author’s ideas. Robert Frank (personal communication 14 May 2002) provides a case in point: his paper (Frank, 1987) “If Homo Economicus Could Choose His Own Utility Function, Would He Want One with a Conscience?” was subsequently published in the *American Economic Review* and originally consisted of two parts. The first part contained what he really wanted to convey to the readers, and the second part was a formal appendix Frank himself did not find of much importance. One of the referees demanded that the first part be deleted, and that the (according to Frank clearly less interesting) appendix essentially constitute the paper. Frank agreed, because he knew that this was the price he had to pay to have the paper accepted by the *AER*. Many such stories are to be heard in our profession.

- (2) Many *preliminary* articles circulating in paper form, or available on the web, are known, appreciated and fruitfully used by other scholars. In those cases, the established refereeing process does not seem to be necessary to spark interest in the academic community. One may even advance the hypothesis that the “censored” versions emerging after passing the refereeing process have less impact.²²
- (3) Non-economists are using the results produced in modern economics and its publication system less and less, because they judge them to be far from relevant.²³ There is substantial evidence that economists have gradually been losing their position as important advisors to governments. *The Economist* (1997: 13; 2000: 90), for example, wonders about the “Puzzling Failure of Economics,” and asks “In the long run, is the subject dead?” or the *New Yorker* (Cassidy, 1996: 50-1) remarks: “. . . a good deal of modern economic theory, even the kind that wins Nobel Prizes, simply does not matter much.” This apparent failure has been reflected on the market for students. In most countries, economists have lost much ground to other disciplines, in particular to management.²⁴

At least according to such evidence, it is not easy to defend the position that the existing journal publication process contributes greatly to making economics a generally relevant, innovative²⁵ and exciting discipline.

Scholars should be treated like artists

Is it possible to avoid intellectual prostitution at all? Is it not simply the reflection of scarcity forcing scholars to adjust? Room for publication, especially in renowned journals, is much smaller than the number of articles that scholars want to place. As elsewhere in the economy, the suppliers of services—here ideas contained in articles—must adjust to the demanders’ wishes. In this sense we are all prostitutes (in which case the term is without any meaning).

But a different view may be entertained. Scholars may be compared to artists, who we expect to express their own original ideas and convictions. We do not simply expect them to produce what the market wants. Production for the market was the rule in the Middle Ages, and painters and musicians were simple artisans who had to do what their customers wanted. But the Renaissance brought a complete reorientation: artists were given the right to express themselves with as few restrictions as possible. The result was an explosion in *creativity*, Leonardo da Vinci and Michelangelo Buonarroti being the best-known examples.²⁶

If scholars are to be original in a similar way to artists today, they have to be given as much independence as possible. This principle has been accorded to universities a long time ago, and is rightly guarded by them. It has also brought about an explosion of innovation never seen before.²⁷ Nevertheless, economics scholars have managed to establish a journal publication system, tending to turn them into intellectual prostitutes.

But how is the scarcity problem inherent in scholarly journal publication to be solved? It is important to acknowledge that many different procedures are feasible and are used in other disciplines. While the present system in economics is similar, for instance, to the one in psychology, it differs strongly from that existing in legal research. The institutional setting of journal publications presently existing in economics is special, because it attributes more or less veto power to the anonymous referees, whose interests are not aligned with those of the journal they advise, and via competition with the overall interests of economics as a discipline. The next section presents an alternative proposal, which seeks to redress this imbalance and provides more scope for individual scholars to be able to express their own ideas rather than to intellectually prostitute themselves.

A proposal to reduce intellectual prostitution

Prostitution, be it sexual or academic, involves choice. It is useful to distinguish between the possibilities individuals have to evade prostitution, given the existing journal editing system, and changing the journal publication system.

Individual options

There are three major ways of reducing the burden of being forced to intellectually prostitute oneself:

- (a) Revising papers according to the demands of referees and editor(s) can be taken as purely instrumental to gain entry and tenure into the academic system. But once in, one stops conforming to the undesirable demands. Given reasonable magnitudes of the relevant costs, benefits and parameters (like the probability of final rejection) in the *Journal Publication Game* of figure 1, this normally means that it is more attractive to turn to publication activities with no, or at least less stringent, refereeing demands. Conventional possibilities are to write and edit books,²⁸ contribute to collections of articles or *Festschriften*,²⁹ or write newspaper columns – or not to publish at all. Increasingly it is possible to put one's articles on the web or in some working paper series (provided there is no refereeing process similar to printed journals).

However, as is the case with many other resolutions, it is not easy to act in a time-consistent way. Once one has entered academia, incentives change. There are moral costs to change course once one has been successful and to refrain from intellectual prostitution. These are the costs imposed by cognitive dissonance. Moreover, acting to please the referees and editors in order to get into the journals is a kind of social capital one does not like to waste. As Oster and Hamermesh (1998) empirically show, success breeds later success, i.e. once one has been able to publish in refereed journals, it is easier to continue. Finally, once one belongs to a group or—as Leijonhufvud (1973) states—to a “caste” (here the tenured professional economists in the universities), one tends to identify with it,³⁰ and finds it costly to deviate from convention. For these cost reasons, it has to be expected that few academics are willing to change once they are in academia.³¹

- (b) One can totally refuse to intellectually prostitute oneself by submitting papers to journals accepting papers without the formal refereeing process. This is almost impossible nowadays because non-refereed journals count little, if anything, with respect to crucial academic career decisions. In tandem, the respective articles must be expected to receive less attention, and to be quoted less.

To many readers, this option probably seems naive, or even ridiculous. But at least one noted economist successfully pursued this strategy.³² He knew that the referees would demand changes he was not prepared to make. He therefore submitted his papers, some of which became “classics”, to non-refereed journals. This risky strategy paid off in the case of Reinhard Selten: he was awarded the Nobel Prize.

- (c) One can fight the demands made by the referees and editors. This is again a risky strategy. Academics tend to be *prima donnas* and do not easily change their conclusions, especially when the refereeing process has cost them much time and effort. To fight back is much easier with formal aspects than with matters of content, which are sometimes mixed up with ideological preconceptions. Fighting back has costs beyond reducing the chance of eventual acceptance. Most scholars also suffer costs when being a nuisance to other persons. There are further costs in terms of self-respect. Some authors are too proud to start haggling about a decision taken by other persons.

Changing the journal publication system

A modest change in the publication procedure would greatly reduce the incidence of intellectual prostitution. The journal’s editor(s) should take the *basic decision* of whether to accept or reject a paper at the outset, based on how interesting they judge the content to be. In some cases, it may be useful to consult other scholars about this basic decision. The appropriate body is the board of editors, which has no function with most journals today.

The board exists to provide academic weight to the journal, but also to serve as a clue for what type of papers the journal is interested in. Under this provision, the managing editor(s) should be able to judge whether a paper’s content is valuable enough to merit publication in the journal. Because the editor(s) (and possibly the members of the board consulted) would only have to decide whether to accept or reject a paper, the decision can be quickly made. This procedure would greatly reduce two types of cost to the prospective authors: the time

cost would be much lower than today, and there would be no prostitution cost, because the acceptance decision would not be conditional on meeting the referees' demands.

Only in the second phase would referees be invited to collaborate. They should solely make suggestions on how the paper can be improved. Then the authors can use what *they* find useful, and can disregard what *they* do not like. The role of the referees would change. Instead of having to mainly demonstrate how clever they are in destroying the author's ideas,³³ they would now be invited to be supportive. The referees would lose some of their power, in particular they could no longer veto a paper they did not like (for instance, because their own ideas are not sufficiently appreciated). As a consequence, there would be an incentive problem. It would become more difficult to get referees. This could be counteracted by giving them the right to publish their dissenting views, together with the original paper.³⁴ The space allotted for this purpose could be short (say one printed page or less).

It might be argued that the difference in procedure would have no effect on the papers published. Editors would choose the papers they like. The born or learned intellectual prostitutes would again win because they are best able to predict what the editors like. But there is a major difference in the result, due to the different property rights to the journal. The anonymous referees, who have no property rights to the journal, might become active only *after* the basic acceptance decision has been made. The editor(s) who would have (some) property rights on the journal due to their reputation, would have no incentive to simply accept those papers they personally like. At least to some extent they would be induced to accept papers expected to raise the journal's reputation.³⁵ They would actively compete for papers they expect to make a future impact on the field; i.e. to be cited much in the future (Laband and Piette 1994). Giving editors full power³⁶ to take the basic decision would thus make a substantial difference to what papers are published.

It may also be claimed that the proposal made is futile because a "good" journal already follows the procedure that the editor(s) decide(s) and the referees only advise. But in most cases,³⁷ this remains at best an ideal and is not put into practice. Generally, the editors can only accept papers if *all* the referees agree *after* one or more rounds of revisions.³⁸ Under the present system, an editor cannot accept a paper he or she likes when the referees advise rejection. Only when the referees' opinions differ and are in clear contradiction, has the editor some discretionary power. Seasoned (sole) editors develop skills to extend their discretionary power, mainly by choosing the "right" referees. But there are clear limits to such attempts. With some journals, the decisions are taken by the group of managing editors. While, with the help of their reputation, they have stronger property rights than outside referees, authors only have a chance if they muster the support of the majority, and do not provoke a veto. This system again creates incentives for intellectual prostitution.

It may further be questioned whether the authors whose papers have been accepted have sufficient incentive to improve their paper. But rational authors still have a strong incentive to integrate good suggestions offered by the referees at the second stage. Only if the paper is well written and up to the standards of the profession will it have any impact on the discipline and be cited. It is therefore in the authors' own best interests to carefully listen to the suggestions for improvement—but this is quite distinct from being "forced" to follow the changes demanded by the referees.

Yet another concern may be that the editors are unable to fulfil the more extensive role attributed to them. The major role of the referees cannot be discarded without loss of quality in publications. This is debatable because, due to the missing property rights, the referees' interests are not in line with the journal seeking their advice. Scholars are asked to referee a paper without knowing anything about the quality of the *other* papers under consideration for publication. Referees thus have to try to decide according to some mystical absolute standards rather than be able to select the *relatively best* paper from those submitted. The editors, in contrast, do have this knowledge and are therefore well equipped to make the basic initial decision.

When judging the proposal advanced here, it should not be overlooked that, according to many observers, the present journal publication system attributing veto power to referees does not function that well.³⁹ Thus, Clower (1989: 23), a former editor of the *American Economic Review*, states: "Much economics is so far removed from anything that remotely resembles the real world that it is often difficult to take the subject seriously." Blaug (2002) is even more scathing: "Modern economics is sick; economics has increasingly become an intellectual game played for its own sake and not for its practical consequences." But whatever one's opinion about the state of economics, a *comparative* perspective is required. No publication procedure, *including* the one existing at present, produces perfect results. It is therefore necessary to compare the imperfect results produced by the modest change in the publication process suggested here to the equally imperfect results of the present system.

According to the analysis provided here, much speaks in favor of letting the editor at the initial stage take a basic decision on whether to accept a paper which has been submitted. At a second stage, only the referees would be invited to make non-binding suggestions for improvement to the author's paper.

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Notes

1. Already at this point, the system makes it most unlikely that this paper would be publishable in an economics journal without fundamental changes. The probability that at least one of the three referees feels offended by the definition of prostitution used is extremely high. It is understandable that many people who have published by meeting the referees' demands therefore dislike being referred to as (academic) prostitutes.
2. That editors reject papers if only one referee advises rejection irrespective of the other referees' judgement is based on the author's own experience, and is supported by confirming impressions by many other economics scholars the author consulted on this point. See also Seidl, Schmidt and Grösche (2002) who take it as a matter of course that a referee has veto power despite the fact that referees' recommendations may be highly contradictory (Cicchetti, 1991). They also refer to much literature establishing the fact for other disciplines.

3. Much prostitution is indeed voluntary and undertaken because of the good pay; see Edlund and Korn (2002: 209–210 and 188–192).
4. See Ellison (2000), who finds on average somewhat shorter periods between initial submission and final acceptance, varying from 3 – 6 months to 2 – 3 years. My impression is that the time until authors get the first reaction has increased considerably. The notion that the reviewing process has lengthened is supported by the empirical evidence collected by Coe and Weinstock (1967), Yohe (1980) and Laband, McCormick and Maloney (1990). A recent (non-representative) survey of the refereeing process of 166 journals in economics (Seidl, Schmidt and Grösche, 2002) asked the question: “After submission, how long did it take on average to get a reply other than just a confirmation that your paper had been received?” It turns out that hardly any journal decides in less than 10 weeks, and more than half need 20 weeks and more to take a decision. The average length is 21 weeks, i.e. more than five months.
5. Here the model could be modified to allow for the fact that the more fully the author follows the demands made by the referees, the higher his likelihood of acceptance is. But the relationship is not straightforward because the referees sometimes make inconsistent demands among themselves, so that the author is required to perform a higher order act of prostitution (it is rare that the disagreements can be exploited in his favor and therewith reduce the extent of prostitution).
6. It is easy to envisage further rounds of revisions.
7. I believe I have some experience and competence in this area. I have published more than 350 papers in over 140 refereed journals during the period 1965–2002. Among them are leading economics journals such as *AER*, *JPE*, *RES*, *REcsStats*, *EJ*, *JEcLit* and *JecPersp.*, but also in political science (e.g. *APSR*), psychology, law and sociology journals. I have also tried the alternatives to journal publications by writing 16 books and by being a columnist for a leading weekly newspaper. I have served as one of the two (and later three) managing editors of *Kyklos* since 1970, am a member of the board of editors of 23 journals and over the years have served as referee for numerous journals.
8. See, for instance, Laband (1990), Blank (1991), Hamermesh (1994), Laband and Piette (1994), Engers and Gans (1998) and Coupé (2000).
9. This distinction has been disregarded in the literature. Laband (1990) is a notable exception, but he does not analyze the behavioral consequences.
10. Only in this case are the editors willing to act. But the only consequence is that they do not send any future papers to the respective persons. This is hardly a serious “punishment” in view of the fact that refereeing involves a substantial amount of work and effort.
11. But it would be interesting to analyze why this is not the case, especially in view of the fact that it is possible to derive sizeable profits from running an academic journal, see Bergstrom (2001).
12. Some journals seek to raise the reputation of individual editors on their board of managing editors by explicitly stating which editor is responsible for having accepted a particular article.
13. A similar process of cognitive adaptation seems to happen to sexual prostitutes, most of whom report no feeling of regret about their choice of occupation (Bullough and Bullough, 1987).
14. That especially the first article published is of great importance for economics’ scholars is shown by Siow (1991).
15. Even Nobel Prize winners are asked by their deans: “What did you publish last year?”
16. Many economics authors are dissatisfied with the refereeing process. In the empirical study by Seidl, Schmidt and Grösche (2002) it turns out that out of 106 professional journals only 36 are considered to perform in a satisfactory or better way with respect to the way they handle submissions. 23 were judged to do so in an unsatisfactory or even bad or very bad way, among them many of the most prestigious journals included in the well-known list compiled by Diamond (1989).
17. It has become quite common for authors to attempt to predict who will be the referees. By citing some authors they expect to be favorable to their paper, they raise the chance of them being chosen by the editor as referee. Such behavior undermines the competitive nature of the intellectual process (see, for instance, Popper, 1994).
18. Gordon Tullock long ago suggested that scholars should be able to recommend papers they find particularly good. If, for instance, a paper is recommended by Kenneth Arrow or William Baumol, a valuable signal is provided to other scholars inducing them to consult the paper. It would, of course, have to be ensured that such recommendations are not forged.
19. This seems to be the case for sexual prostitution because otherwise it is difficult to explain the large demand in all periods and countries (for evidence see Edlund and Korn, 2002).

20. But even among extremely successful economists, awarded the Nobel Prize, there are some who harshly criticize the existing journal publication system. Examples are Leontief (1971), Coase (1994) or Buchanan (2000) see more generally Leijonhufvud (1973) and Cassidy (1996).
21. But Laband (1990) explicitly warns the readers in his second footnote: "The search for value-added necessarily is grounded on the *assumption* that the review process in economics is intended to add value to manuscripts." (my emphasis).
22. This hypothesis is in principle empirically testable. One could, for example, study the development over time of the proportion of censored papers to uncensored papers quoted by other scholars in their work. But this indicator is difficult to construct because many papers appearing in working paper series on paper and the web had to pass some sort of refereeing process. Moreover, scholars possibly quote unpublished papers less than published ones, and this tendency may have increased over time. This is not necessarily due to an effort to appear more original than one really is, but perhaps even more to the fact that one remembers an unpublished paper less vividly than a published one when sitting down to write an article.
23. This view is likely to invite the veto of at least one, if not all, of the referees. Most academic economists do not share this evaluation at all, but rather side with Summers (2000: 1), proclaiming that "What economists think, say, and do has profound implications for the lives of literally billions of their fellow citizens" or Hamermesh (personal communication to the author by e-mail 27 May 2002) stating "We (economists) have never had more influence at the top; we are the new priesthood." It is, of course, not surprising that economists, like any other self-interested group, fight for their profession and tend to reject criticism.
24. These aspects are more fully discussed in e.g. Stigler (1982), Clower (1989), Blaug (2002), van Bergeijk et al. (1997), Middleton (1998), Reder (1999) and Frey (2002) where a large amount of literature is quoted.
25. It has often been stated that it is very difficult to publish unorthodox papers under the present journal publication system; see, for instance, the evidence collected in Gans and Shepherd (1994).
26. I here draw on the literature on the economics of art; see Throsby (1994, 2000), Towse (1997), Frey and Pommerehne (1989) and Frey (2000).
27. Commercially driven research within firms may also be innovative, but it is characteristic that such institutions often act as if they were universities, leaving their researchers considerable freedom.
28. But more and more editing houses ask outside referees to evaluate book proposals and manuscripts. Normally, referees interfere here much less than in journal publishing, especially when it comes to evaluating a completed manuscript. Therefore, with books, the need to intellectually prostitute oneself is considerably lower.

But some editing houses find it a good idea to ask referees to be as stringent as for papers in journals. The present author has, for example, been asked by a leading American editing house to evaluate the book proposal by a Nobel Prize winner (Gary Becker). I wrote back that the Nobel Prize winner in question did not stop or even reduce his intellectual pursuits after being "nobeled" and that they should therefore publish the book. The editors were most angry about this answer. Obviously they expected me to evaluate whether a Nobel Prize winner should be able to publish a book. But would this not be pure hubris on my part (and on the part of most other possible referees)? What is the point then of refereeing books in that manner?
29. Established economists often have good personal contacts to editors. This is normally the case for in-house journals. It has been argued that this helps them to get their articles through with less changes demanded, and allows them to publish in invited special issues. Gans and Shepherd (1994), reprinted in Gans (2000: 34) report: "Editors at several journals apparently sometimes permit certain authors to bypass the journals' normal refereeing process" (see also Gerrity and McKenzie, 1978; Laband, 1985).
30. Leijonhufvud (1973) suggests that economists are solely motivated by peer acceptance.
31. There is an analogy here to sexual prostitution: many women forced into prostitution in order to be able to pay off their debts choose to stay in the profession even when they are debt-free (Muroi and Sasaki, 1997, for Thai prostitutes in Japan).
32. Personal communication to the author.
33. Anonymity certainly contributes to referees being very critical. See the controlled experiments made in the late 1980s at the *American Economic Review* by Blank (1991), finding lower acceptance rates and more critical reviews when both the referees' and the author's identities are unknown (double-blind refereeing).
34. I owe this idea to Reto Jegen.
35. The question of how editors are to be selected is beyond the scope of this paper. It must suffice to state that it would be mistaken to rely on performance compensation, for the reasons given in Frey (1997) and Frey

and Osterloh (2002). Journal reputation is extensively discussed, e.g. in Laband and Piette (1994) or Stigler, Stigler and Friedland (1995).

36. An editor would have full power if the journal belonged to him or her. It would go too far to discuss why there are few, if any, private journals run for profit in economics.
37. As always there are exceptions—at least to some extent. Thus, Dan Hamermesh, as editor of the *Journal of Population Economics*, writes me (e-mail of 27 May 2002): “I must get reports; but if I’m pretty sure I’m going to want the paper, I only solicit one referee. Even if I get two, and even if they’re both fairly positive (. . .), if I’m convinced that it is unimportant, I’ll reject it.”
38. The editors of some journals may reject papers clearly below the journal’s standards without consulting the referees. But most journals do not reject without adding lengthy referee comments. This looks “fair” but disregards the (often high) cost of having to wait for this decision (nowadays often one year or even longer).
39. See the responses from 140 leading economists about their journal submission experiences and the list of “classic” papers once, and often more than once, rejected in Gans and Shepherd (1994). A well-known example is Akerlof’s “Market for Lemons”, which was rejected by the *American Economic Review* and the *Review of Economic Studies* as being “trivial”, and by the *Journal of Political Economy* for being “too general” before it was accepted by the *Quarterly Journal of Economics*, which was instrumental in him winning the Nobel Prize.

In response to the shortcomings of the present journal publishing system some preliminary efforts have been undertaken to improve it. An example is the Berkeley Electronic Journals in Economic Analysis and Policy which guarantees a 10-week turnaround, simultaneous consideration in four journals of different quality levels, and immediate publication after acceptance.

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